

# PRETRANSITIONAL POPULATION CONTROL AND EQUILIBRIUM

**John C. Caldwell**, Australian National University

**Bruce K. Caldwell**, National Centre for Epidemiology and Population Health, Australian National University

**Abstract.** A persistent theme in much anthropological writing is the concept of the deliberate control of population numbers by hunter-gatherers so as to achieve moderate family size, adequate nutrition and constrained adult mortality. This paper examines the mix of theory and field evidence leading to this conclusion and finds the case not proven. It argues, on the contrary, that Malthusian constraints can operate, and probably did operate, to produce a society where most adults were reasonably robust and healthy even though child mortality was high and life expectancy short. It draws attention to the fact that the absence of population limitation in pre-Neolithic times implies high mortality as well as high fertility, and weakens the argument positing a Neolithic mortality crisis.

On the issue of the cultural control of family size before the fertility transition, most demographers and many of those cultural anthropologists with whose work they are most likely to be familiar appear to disagree. Van de Walle (1968b, p. 489) wrote: 'Control of marital fertility by contraception, as we know it today in Western countries, is without doubt, a fairly recent development'. He went on to conclude that the typical pattern for most of the world's population had for aeons been one of early marriage followed by uncontrolled fertility. Knodel (1977, p. 242) decided that birth control must have been adopted by the vast majority of mankind only fairly recently and that its practice was innovative.

Some anthropologists and anthropological demographers have sounded a different note, louder in the late 1960s and 1970s but still influential. Handwerker (1983, p. 5) reported that most anthropologists assume there to have been low birth and death rates through most of human history. Polgar (1971b, p. 3) argued that 'There are several lines of evidence indicating that the voluntary regulation of family size may well have been one of the earliest features of human culture'. Greenhalgh (1995b, p. 15) wrote: 'people without access to modern contraception take steps to limit family size'. Bledsoe and Camara (1997), with their emphasis on Gambian women ensuring that too many pregnancies, births and children did not weaken them, implied that there must have ancient ways of controlling this situation.

This review will attempt to show how it was possible that such contrasting conclusions could have been reached from the same human experience, and will search for a resolution. Although some theoretical anthropological ecologists and biologists assert that this debate is now history (see Maynard Smith 1964, 1976; Wood 1998), prominent field anthropologists often disagree. Cowgill (1988, p. 122), countering Wood's (1998) claim that belief in pre-modern fertility control was now extinct, cited Bledsoe and Camara (1997), and the contributors to Greenhalgh (1995a) and Kertzer and Fricke (1997).

There are some obvious reasons for the different conclusions, but they are far from the whole story. Anthropologists tend to focus on the limitation of population and family size, which includes abortion as well as contraception, and infanticide as well as fertility control. Indeed, demographers, if they could obtain the data, would count infanticide on both sides of the vital-events ledger, as births contributing to fertility and as deaths contributing to mortality. In addition, as Weiss (1976, p. 351) charged, demographers are usually not interested in small societies. Anthropologists concerned with long-term fertility patterns focus on hunter-gatherers who now make up an insignificant proportion of the world's population, whilst in 10,000 B.C. they constituted the total, although only

about 10 million altogether (Lee and DeVore 1968, p. 5 and frontispiece). Accordingly, the present-day survivals are used to provide evidence about how their distant ancestors behaved, an approach with significant dangers.

In contrast, anthropologists and demographers are in agreement that, for most of human history, populations have been over the longer term very close to being stationary or in a condition of equilibrium. Birth rates and death rates have been almost identical. They have little choice but to agree because the mathematics of exponential growth show that any persistent margin of birth over death rates would have resulted in far greater population growth than has actually occurred. Most demographers assume that this balance of births and deaths was Malthusian with mortality holding down fertility's potential for population growth. Most anthropologists rarely mention Thomas Robert Malthus (1766–1834) except to claim that premodern man was sufficiently resourceful to bypass his constraints. The neglect of Malthus when focusing on premodern populations is unfortunate, as Malthus in 1798 in the first edition of his *Essay* analysed the nature of this age-old equilibrium. He postulated that man needed food, the production of which had grown slowly, if at all, for most of human history. He also postulated a constant passion between the sexes which normally led to so many births that population would have grown had it not been constrained by mortality driven by the limited food supply.

Since the beginning of the anthropological analysis of population stasis, essentially starting with the biologist, Carr-Saunders (1922), this has often been taken to mean that a population in Malthusian equilibrium was constantly on the verge of starvation, weakening both the people and their society. But this is clearly not what Malthus meant nor what his type of equilibrium would imply. One reason is that much of the mortality constraint on premodern hunter-gatherers took the form of violent deaths, mostly in warfare, and possibly accounting for as much as 30 per cent of male mortality (Coleman 1986, p. 29). That deaths unconnected with starvation may protect the rest of the society is conceded by those anthropologists with a focus on infanticide. The second reason is that Malthus believed, not that the mortality arising from insufficient food usually took the form of starvation except in famine crises, but that the lower average nourishment was, the more likely were people to die of infection, especially during epidemics. This is an argument that McKeown (1976) has applied to England into the twentieth century and that Fogel (1997) is beginning to use for even later dates. It is only in recent times that we have been in a position to control infection without raising nutritional levels. Thirdly, Malthus believed that much of the high mortality needed to constrain growth was normally borne by specific, and not very visible, sections of the population without affecting the rest of society. He pointed to two groups. One was the lowest class, which he regarded as less worthy, often unemployable, and given to such reprehensible demographic behaviour as marrying early and threatening to have large families. Indeed, he felt that any help to this class would lead the constraint of high mortality to impinge on the class next above it, consisting of worthy artisans and skilled workmen. The other section of society which felt the full ravages of the constraining mortality consisted of young children, on whom we might suppose the impact paralleled that of contraception or infanticide. Rose (1968) has described such a society as late as 1941 in Groote Eylandt off Australia's northern coast, with slow population growth and adults appearing fairly robust and healthy while a large proportion of the young children was wiped out by very high mortality. Malthus (1959, p. 15) maintained that conditions in such societies prevented 'any but the most robust infants growing to maturity', and cautioned that 'we must not fix our eyes only on the warrior in the prime of life'. Finally, in the later modifications of the *First Essay* he pointed increasingly to the benefits of Western Europe's postponement or forgoing of marriage, especially among women, as allowing its population to escape the miseries of societies in the rest of the world, such as the early-marrying Chinese. He clearly was not saying that Europeans had escaped the constraints of a slowly growing food supply but that they had achieved an

equilibrium between population and food at a more comfortable and demographically less oppressive level of living.

Recent research has thrown light on different levels of the Malthusian equilibrium, especially the construction of stable population tables. With women averaging 6–7 births, stationary population is achieved with an expectation of life at birth of 20 years, around 50 per cent of births resulting in deaths by five years of age and 50 per cent of all deaths in the society being to those under five years of age; with 4–5 births per woman, a life expectancy of 30 years, and 40 per cent dying by five; and with just under four births, an expectancy of 40 years and 30 per cent dying by five. This can be compared with the contemporary developed world where a stationary population is achieved with a life expectancy of almost 80 years, just over two children per woman, and one per cent of these births succumbing to death in their first five years (Coale and Demeny 1966, West tables). It is not suggested that premodern populations could attain a life expectancy of 80 years by restricting family size to two children, but they might have achieved 30 years while averaging less than five births or possibly 40 years with under four births. This is exactly the mortality level that many anthropologists believe most premodern hunter-gatherers achieved by limiting, if not their births, then those children who survived infanticide to an average of around four per woman.

Modern research agrees that Western Europe for centuries limited fertility by women postponing or forgoing marriage. Hajnal (1965, p. 132) toyed with the idea that the emergence of modern Europe and capitalism resulted from the European marriage pattern, a view with which Malthus seems to have concurred. Goldstone (1986) used evidence on England from Wrigley and Schofield (1981) to show that the major influence on fertility was lifelong spinsterhood. Over more than three centuries, 1551–1875, marital fertility levels scarcely changed with overall fertility being determined solely by fluctuations in marriage (Wilson and Woods 1991, pp. 403–404). There is little in the way of long series of premodern demographic measures but the reconstitution by Wrigley and Schofield (1981) does provide a test for England. Between 1650 and 1750 the population was close to stationary, growing by less than ten per cent over one hundred years, with the crude birth rate averaging only one point per thousand population above the death rate. This was achieved with an average total fertility rate (i.e. the average number of births per woman over a lifetime if demographic rates change little) of 4.6 and life expectancy at birth of 37 years (Wrigley and Schofield 1981, p. 230). The conditions were different from those of hunters and gatherers: society was more organized and sporadic killing much less common, but closer settlement, especially in the towns, meant a greater load of infection. Nevertheless, the Wrigley and Schofield figures do demonstrate that an average of less than five births per woman (a crude birth rate in the 30s) is compatible with a life expectancy of nearly 40 years and indeed probably causes that expectancy to be so high. This is evidence of more food per head and less pressure on resources, even though it is probably still the availability of food which determines the society's level of resistance to mortal illness. It is a moot point whether, if fertility had fallen any lower, life expectancy could have climbed further, thus preventing population decline. It might be noted, however, that by 1787 Denmark was recording a total fertility rate around four and a life expectancy of 43 years (Coale 1986, p. 6, fig.1.2).

We will now examine the differing experiences and traditions of those demographers and anthropologists involved in the issue which led them to widely separate understandings of the nature of pretransitional population control and population equilibrium.

### The demographers' experience

Twentieth-century demographers were at first far from sure that contraception was a relatively recent innovation. Many were influenced by Himes's (1936) *Medical History of Contraception* which documented the use of *coitus interruptus*, sponges, douching, suppositories, and herbs over the millennia, and condoms, quinine, vinegar, and alum over recent centuries. In his introduction, Himes drew mainly on Carr-Saunders (1922) but also on Sumner *et al.* (1927), stating: 'Man's attempts to control the increase in his numbers reach so far back into the dim past that it is impossible to discern their real origin. Some forms of limitation on the rate of increase are undoubtedly as old as the history of man' (Himes 1936, p. 3). But later in the book, after having to search vast quantities of literature for relatively rare references to contraception, he seemed less certain of a demographic impact and did not refer again to any significant effect on society's numbers. Instead, he more cautiously concluded: 'The desire to control conception is a well-nigh universal culture-trait, universal, that is, in time and space. The *desire*, often unconscious, is much more universal and general than the *practice*' (Himes 1936, p. 421, his emphases).

One channel of Himes's influence on demographers was through Frank Notestein, head of Princeton University's Office of Population Research (OPR). In his key writings, which usually drew largely upon OPR sources, Himes was one of Notestein's two most frequently cited outside sources (Caldwell 2001, p. 10,750). Notestein was strengthened in his belief that *coitus interruptus* had long been known and available to the human race by his questioning of immigrant Europeans at a New York birth control clinic about contraceptive methods they had earlier known and used in Europe (Stix and Notestein 1940, p. 150).

Over the last 50 years demographers' scepticism as to pretransitional fertility control of sufficient magnitude to lower societal fertility has been driven by large-scale demographic surveys such as those carried out in sub-Saharan Africa by French statistical and technical aid agencies from the mid-1950s, the KAP (Knowledge, Attitudes and Practices towards Family Planning) surveys from the 1960s, the World Fertility Surveys (WFS) from the 1970s, and the Demographic and Health Surveys (DHS) from the 1980s. Where they were able to examine completely pretransitional populations, mostly in sub-Saharan Africa, fertility levels were usually found to be very high and reported knowledge or practice of contraception very low. In one of the earliest surveys, that in Guinea in 1954–55, the demographic levels for the little-developed Forest *Département* were crude birth and death rates of 45 and 42, almost balancing, and an infant mortality rate over 250 with almost half of all births followed by deaths within the first five years. The implied life expectancy was around 27 years. In Upper Guinea the total fertility rate was 6.2 with birth and death rates of 50 and 42 (Coale and Lorimer 1968, p. 157). As the surveys of sub-Saharan Africa continued, the recording of total fertility rates of 6–7 became typical.

The scepticism of the existence of universal population control was increased by Henry's 1961 paper with its concept of 'natural fertility', and by later publications on the same subject (see Leridon and Menken 1979). Henry argued that natural fertility was not always at the same level because of differing durations of postpartum lactation and sexual abstinence, but that any conscious limitation of family size would inevitably be detected by a steeper fall in fertility in the late reproductive period as families grew in size than where there was no such control. He drew on pretransitional records from Europe, Asia and Quebec, as well as more recent information from Guinea (Fouta-Djallon) and the non-contracepting American Hutterites to show age-specific fertility curves of the same shape, and concluded: 'For non-European populations there is often strong evidence to suggest virtually complete absence of birth control...' (Henry 1961, p. 81). This demonstration was reinforced by the development of similar families of curves for age patterns of marriage (Coale 1971) and model fertility schedules (Coale and Trussell 1974, 1978). Particular

attention was given to the non-contracepting Hutterite religious community in the United States, first by Eaton and Mayer (1954), and then by Coale (1973) when designing the basic measures underlying the Princeton European Fertility Project. The Hutterites exhibited very high fertility because they were well nourished and had shortened the duration of their breastfeeding period. Furthermore, Coale showed that theoretically their fertility could be higher still if they did not breastfeed at all. Mistakenly, Abernethy (1979, p. 12) and other anthropologists have taken the fact that the fertility of other pretransitional populations is lower than that of the Hutterites to argue that all these other societies must therefore be consciously limiting fertility.

Most demographers were even more convinced of pretransitional natural fertility by the application of a statistical measure, 'm', by Coale and Trussell (1974) which measured the distortion of the natural fertility curve caused by birth control. Knodel (1977) employed both this measure and the shape of age-specific fertility curves to conclude that at the onset of the fertility transition birth control must have been innovative for the vast majority of the population involved, and maintained that his approach disproved Carlsson's (1966) conclusion of the pre-existing knowledge of contraceptive methods (Knodel 1977, pp. 240–247).

Finally, researchers working in the anthropological demography tradition, usually combining anthropological approaches with small-scale surveys, persistently failed to find evidence of fertility control in sub-Saharan Africa and South and Southeast Asia before the advent of modern family planning movements. This was the case in sub-Saharan Africa (Caldwell and Igun 1970; Orubuloye 1981; Caldwell and Caldwell 2000a, b), South India (Caldwell and Caldwell 1984), Bangladesh (Duza and Nag 1993) and Thailand (Knodel *et al.* 1987). All reported that the first generation of contraceptors testified that they would neither have thought of the possibility of controlling fertility nor have attempted to do so but for their contact with family planners and family planning programs. They also said that the absence of contraceptive provision was why their parents had not practised birth control.

The only study of hunter-gatherers with longitudinal demographic data was that by Howell (1979) of the Dobe !Kung in the Kalahari Desert in Botswana close to the Namibia border. As affluent-society anthropologists might have predicted, they exhibited only moderate fertility: a total fertility rate of 4.7, a Princeton marital fertility index ( $I_g$ ) of 0.4 and a total fertility index ( $I_f$ ) of 0.33. One-third of the fertility difference between the !Kung and the Hutterites was caused by sterility (Howell 1979, p. 166), probably with sexually transmitted disease being a factor. Howell (1986) reported having found no contraception or abortion among the !Kung, but suspected that there might have been a low level of undetectable infanticide, a supposition based mainly on the arguments of those anthropologists insisting on its universality among such people (p. 182).

### **An opposing anthropological tradition**

The opposing anthropological tradition emerged almost fully formed in a 1922 book by the English biologist, demographer, social scientist, and educator, Alexander Carr-Saunders. That book, *The Population Problem: A Study in Human Evolution*, was the main source of the theory of age-old population control as well as that of hunter-gatherer affluence (see Sahlins 1974). Himes (1936) described Carr-Saunders's book as an excellent study and drew almost alone upon it in the conceptual section of his *The Medical History of Contraception* (1936, p. 3). Wynne-Edwards (1962, pp. 21–22), who first described social homeostasis, warmly praised Carr-Saunders as the pioneer in the field and regretted only that he had not discovered Carr-Saunders (1922) until he had finished the first draft of his own book. Carr-Saunders (1922) recognized as his specific predecessors the optimum population theorists, especially Edward Cannan, the British economist

who, in *Wealth* (1914), developed the theory of 'optimum population', which argued that there was one level of population density which maximized human per income per head.

Carr-Saunders's (1922, pp. 230ff.) starting point was that of the fit hunter and his adequately fed relatives, people very different from Herbert Spencer's description of pitiful populations restricted by mortality and always near starvation (Spencer 1899 [1864] Vol. II, p. 15). Carr-Saunders cited descriptions of the Australian Aborigines by field researchers, especially Spencer and Gillen (1938 [1899]) and Schürmann (1879), and others of the Bushmen of Southern Africa, Amerindians, and forest peoples of Southeast Asia. He maintained that their families were small (Carr-Saunders 1922, pp. 135ff) and that man is intelligent and has always been able to keep fertility below fecundity (p. 52), so that 'some approach to the optimum [living standard] is everywhere attained' (p. 231). In addition overpopulation and misery had to be avoided because they led to social disorganization (p. 213). This restriction was the result of the conscious regulation of numbers (pp. 197 ff.), achieved by postpartum sexual abstinence, prolonged breastfeeding, warfare, neglect of children, abortion, and infanticide (pp. 213 ff.). The supporting evidence was not easily obtained because, with the coming of Europeans, mortality became so high that the hunter-gatherers abandoned female sexual abstinence, abortion, and infanticide, especially as missionaries and colonial law were against the latter two. The 'historical races' (i.e. the agricultural societies), being more densely settled, were characterized by higher mortality and hence had less need of fertility control, although there was some contraception and perhaps more abortion and infanticide (pp. 243–263). These arguments were supported by references to nearly a thousand publications of very varying quality.

The arguments that were to be put forward repeatedly over the next eight decades were all there. In his 1934 book, *Primitive Society and Its Vital Statistics*, Krzywicki, the Polish anthropologist, took them for granted. The equality of cultures and the demonstration that human beings had always been in control of their fate were appealing to many anthropologists. The demonstration that the evidence was likely to disappear before its would-be chroniclers could record it made the case almost irrefutable. Nevertheless, the pursuit of evidence was to follow, especially among those with the least Western contact, Eskimos and Australian Aborigines. The major modification was that, as researchers discovered little mass use of effective contraception and that abortion had proved difficult and dangerous in premodern societies, the means of population control was increasingly identified as infanticide, child abandonment, child neglect, and in some groups suicide among the elderly. Infanticide and child neglect were difficult to prove in high-mortality populations except where they were sex-selective and hence where there was evidence of much greater female than male child mortality. Attention remained concentrated on hunter-gatherers, and little note was paid to agrarian Japan, China or parts of North India where there was ample evidence that infanticide was often practised at least during famine, producing much the same effect as that of deferred marriage in Europe.

Weyer (1962), in a book on the North American Eskimos first published in 1932, drew on his own findings during a 1928 expedition, and many other sources, to write a chapter on population control (Weyer 1962, pp. 131–171) where he specifically acknowledged Carr-Saunders's theoretical guidance. He reported little abortion or contraception, but significant levels of infanticide, supplemented by some killing of the aged and infirm. He supported his contention that infanticide was widely practised with data showing 46–92 per cent as many girls as boys in 13 Canadian Eskimo groups, but pointed also to a female adult excess caused, he claimed, by very high male mortality when hunting. He did not consider the possibility of sex differentials in age misstatement. Significantly, he denied group pressure or direction (except in the sense that infanticide was tolerated), or any aim of optimizing population levels, but attributed killings to individual families faced by specific crises such as famine in a harsh environment or the need for mobility frustrated by a mother bearing a second child while the first could not travel without help.

Firth (1936), in a book resulting from field work in the early 1930s on Tikopia, an isolated Polynesian island in the Western Pacific without any European residents, included a chapter on ‘Modern population problems’ (pp. 408–418). His work was the closest to fitting fully Carr-Saunders’s (1922) model, although Carr-Saunders was not referenced even though he was Director of The London School of Economics where Firth worked. Firth claimed that the islanders were prosperous with remarkable health (Firth 1936, p. 411) and attributed this to population control achieved by infanticide and assisted by the occasional high mortality from warfare and sea voyaging. He believed that there had probably also been some use of abortion and *coitus interruptus*. Knowledge of these practices, and easy proof that they had been employed, had largely disappeared because of the colonial government (of the Solomon Islands) and Christian missions. Nevertheless, Firth concluded that there was much to be regretted in the passing of the old ways: ‘A celibacy in which chastity was not enforced, and a discreet infanticide, would serve to maintain the population in equilibrium, and would be in accord with the feeling of the people themselves’ (Firth 1936, p. 417).

Balikci (1967, 1970) reported on the Canadian Eskimos drawing on his own field work in the 1960s and, for the earlier precontact period, on the relatively inaccessible reports of Knud Rasmussen (1931) and data from the beginning of the twentieth century assembled by Weyer (1962) on sex ratios among children. Balikci (1967, p. 622) concluded that ‘Female infanticide is a vital part of the long range “family” population policy’. Infanticide was based on *ad hoc* decisions within the family, there being no fixed rules, and no community involvement other than that the practice was accepted. Later he concluded that the reasons were food shortages and, in the case of female infanticide, the need to balance the numbers of the sexes in the adult marriage market, depleted by high male mortality during hunting (Balikci 1970, pp. 150–153). In the last 30 years Balikci has become the most frequently cited authority on Eskimo population control although nearly all his population control data are drawn from earlier sources. Langer (1974a, b) drew together two millennia of references to infanticide in Europe, but without numbers, and with his references to hunters and gatherers restricted to Balikci’s 1970 work (Langer 1974a, pp. 353–354). Others, like Hassan (1981), argued that we would always have difficulty glimpsing mankind’s long history of fertility control because with the advent of agriculture that control weakened and fertility rose as a response to mortality rising as infectious disease spread more easily through dense populations.

### **The evolutionists appear**

The perspectives of some researchers changed from 1962 when Wynne-Edwards published his *Animal Dispersion in Relation to Social Behaviour* that altered the interpretation of fertility limitation from the social to the biological sphere, as the inevitable and largely involuntary outcomes of evolution, and replaced the individual by the community and species. He praised Carr-Saunders as the pioneer (Wynne-Edwards 1962, pp. 21–22), but chided him for beginning with organized human groups or communities in the Lower Palaeolithic instead of going back to protozoan behaviour in the Lower Cambrian (Wynne-Edwards 1962, p. 21). Such praise sits awkwardly with Carr-Saunders’s (1922, p. 52) insistence that the far-sighted innovation of population limitation demonstrated the high level of human intelligence and rational thinking. Wynne-Edwards (1962) invented the concept of ‘homeostasis’ in populations, ‘the general hypothesis of the self-limitation of animals’ (p. 9). ‘Homeostasis’, in a physiological sense, was coined by Cannon (1939) in 1932 to describe the stabilizing regulatory mechanisms of the body (Kunitz 2002, pp. 724, 728). Wynne-Edwards (1962, pp. 21–22) approvingly echoed Carr-Saunders (1922, pp. 200, 213): ‘every population has an optimum number, or an optimum density, that enables the greatest income per head to be earned; and above this density returns diminish’. Wynne-Edwards’s interpretation was:

Judging by appearances, chronic over-exploitation and mass poverty intrude themselves on a mutually-balanced and thriving natural world only as a kind of adventitious disease, almost certain to be swiftly suppressed by natural selection... if each species maintains an optimum population-density on its own account, not only will it be providing the most favourable conditions for its own survival, but it will automatically offer the best possible living to species higher up the chain that depend on it for food... it must be highly advantageous to survival, and thus strongly favoured by selection, for animal species (1) to control their own population densities, and (2) to keep them as near as possible to the optimum level for each habitat they occupy (Wynne-Edwards 1962, p. 9).

Nowhere in his large volume does he mention Malthus.

Wynne-Edwards's work has been savaged by mathematical geneticists and population biology theorists with a similar bent on the grounds that his concept of 'group selection' is theoretically untenable, in contrast to evolutionary 'individual selection' or possibly 'kin selection' (Maynard Smith 1964, 1976; Wood 1998).

In spite of Wynne-Edwards largely dispensing with human thought and decision-making in favour of the biological hidden hand, and downplaying the community and society, many anthropologists took readily to his approach. Some liked his greater emphasis than Carr-Saunders on the preservation of resources or what came later to be called 'sustainable development'. The most influential was probably Mary Douglas (1966) who drew heavily on both Carr-Saunders and Wynne-Edwards. She interpreted Wynne-Edwards as positing 'that in primitive groups social conventions operate homeostatic controls on population' (Douglas 1966, p. 273). 'There appears to be density-dependent brakes which impose a ceiling on natural increase. It is important to the argument that the relevant ceiling is *not* imposed by starvation or by predators or natural hazards. Rather it is imposed by otherwise inexplicable aspects of social behaviour' (p. 264, her emphasis). She criticized both Wynne-Edwards and Carr-Saunders for assuming that all populations live in optimal conditions and for excluding underpopulation even though many societies studied by anthropologists appear to be underpopulated (p. 265). She then proposed her own thesis modifying what she took to be agreement between Carr-Saunders and Wynne-Edwards: 'I am going to argue that it is the demand for oysters and champagne, not for the basic bread and butter, that trigger off social conventions which hold human populations down.... My argument is that human groups do make attempts to control their populations, often (but not always) successful attempts. But they are more often inspired by concern for scarce social resources, for objects giving status and prestige, than by concern for dwindling basic resources' (Douglas 1966, pp. 267–268).

She said that Firth missed the central point: that the people of Tikopia or at least their elites, were not merely trying to eat well but were aiming at maximizing their control of the valuable coconut cream which played a central role in festivals (Douglas 1966, pp. 269–270).

Douglas's endorsement of the theories of both Carr-Saunders and Wynne-Edwards has been influential. Her own modification has been much less so. It has at least three problems. The first is that it is difficult to see how the optimum population for maximizing food consumption should not also be that for acquiring coconut cream. The second problem is that she appears to be suggesting that the society as a whole, and certainly the poor, are living below optimum standards, with consequent higher death rates, than need be the case, just so the powerful can acquire more luxuries. This is probably an accurate description of socially stratified agricultural societies, and possibly of Tikopia, but can it possibly be true of hunter-gatherers? The third problem is that in non-stratified societies, such as those found until recently in sub-Saharan Africa, large families

appear to be able to put pressure on small families and consequently to achieve greater economic success (Imoagene 1976; Mendonsa 1977; Caldwell 1981, pp. 6–7).

A much cited work in the same vein (but not referring to Carr-Saunders, Wynne-Edwards or Douglas) was Joseph Birdsell's (1968) 'Some predictions for the Pleistocene based on equilibrium systems among recent hunter-gatherers', which drew on his own research among Australian Aborigines and on his construction of models. He concluded: 'Among even the simplest of peoples, social factors seem to operate to stabilize population numbers below the absolute level of saturation... the expanding population may begin to bud off when values from 60 to 70 per cent of carrying capacity are realized' (Birdsell 1968, p. 230). This was achieved by 'Systematic infanticide [which] has been a necessary procedure for spacing human children, presumably beginning after man's entry into the niche of bipedalism, and lasting until the development of advanced agriculture. It involved between 15 and 50 percent of the total number of births. Among recent hunters it tends to be preferentially female in character and probably was in the Pleistocene' (p. 239).

Neel (1970), a geneticist who analysed anthropologists' research among the Yanomamo of the Venezuela-Brazil border country, found that the Yanomamo had a similar survival rate to fifty-five years of age to that of the people of India in 1901, because the former experienced high adult mortality in warfare, thus reducing them to agrarian mortality levels. He introduced the idea of humane, voluntary and adaptive infanticide in contrast to what might be termed 'Malthusian' infanticide: 'The relationship between rapid reproduction and high infant mortality has been apparent for centuries. During this time we have condoned in ourselves a reproductive pattern which (through weaning diarrhoea and malnutrition) has contributed, for large numbers of children, to a more agonizing 'natural' demise than that resulting from infanticide. Moreover, this reproductive pattern has condemned many of the surviving children to a marginal diet inconsistent with full physical and mental development' (Neel 1970, p. 817). He named Firth (1936) as his inspiration. Dickerman (1975) in a review paper argued that infanticide was mostly practised by hunter-gatherers, horticulturalists and stratified societies both to raise living standards and to maintain the social structure. The awkward issue of the near-absence of infanticide among the unstratified shifting cultivators of sub-Saharan Africa was thus avoided.

Another influential and quotable contributor to the debate was Virginia Abernethy with *Population Pressure and Cultural Adjustment* (1979). This was largely a synthesis of previous theses, especially those of Carr-Saunders (1922) and Wynne-Edwards (1962). In addition she cited Hutterite fertility to argue that nearly all other fertility was constrained (Abernethy 1979, p. 12), and put forward the view that the main trigger for fertility control was the fear of sharing limited resources within a large family (pp. 25–26). Dickerman (1984) argued that infanticide was not well defined and should include neglect extending far beyond infancy. It should be noted that if this is done the distinction between the concept of Malthusian mechanisms and homeostatic equilibria is greatly blurred.

Many anthropologists, demographers, and others who had associated themselves with the new national family planning programs established increasingly from the 1960s were keen to show that these programs were not foisting upon the peoples of developing countries the practices of the West but were offering a means of pursuing ancient behavioural patterns temporarily disrupted by Western intrusion. The high-water mark of this movement was probably *Culture and Population* edited by Polgar (1971a) with 21 well-known contributors, mainly anthropologists. In the introduction he drew on Birdsell (1968), Devereux, Firth and his own work to state: 'voluntary reduction of family size seems a cultural pattern of very great antiquity, and the high population growth rates in developing countries today do not simply result from encrusted "motivations" for

high natality. They are instead partly derived from direct or indirect birth-promoting effects of Western expansion and colonialism during the last 400 years' (Polgar 1971b, p. 6). He justified the last argument from Geertz's (1963) evidence in *Agricultural Involvement* on rising birth rates among the nineteenth-century Javanese sugar plantation workers (Polgar 1971b, p. 4). Polgar argued that the older tradition of birth control could be used by modern family planning programs seeking to curb fertility (pp. 6–7). Later, Polgar (1977, p. 356) wrote that premodern 'Cultures that did not moderate reproduction sooner or later became extinct, leaving the field to those who did regulate their populations'. The claim here is that all surviving cultures had controlled fertility until Christian missionaries and European colonial governments destroyed the system or even encouraged higher fertility as in Java (Polgar 1977, pp. 357–358).

Boserup (1965), in an influential book arguing that population growth was not a continuing potential disaster for premodern man but the mechanism whereby more successful methods of intensifying subsistence production came into use, displayed little regard for either population control or Malthusian limits: 'the low rates of population growth found (until recently) in pre-industrial communities cannot be explained as the result of insufficient food supplies due to overpopulation, and we must leave more room for other factors in the explanation of demographic trends..., medical, biological, political etc.' (Boserup 1965, p. 14). This stance undermined her own approach because the explanation for the almost exact population equilibrium is insufficient. Nevertheless, it is quite possible to argue that Malthusian bonds usually prevailed but that occasionally, at times of population pressure, an innovation in food production was adopted on a sufficient scale to ensure survival and ultimately to help change the world (Caldwell 1986; Lee 1986).

Another influential synthesizer was Sahlins (1974), who, in *Stone Age Economics* (first published in 1972) and especially in his first chapter, 'The original affluent society' (first published in a shorter form in French in 1968), echoes both Carr-Saunders (1922) and Boserup (1965) although he cited neither of them. He argued that hunters and gatherers had relatively low fertility and mortality rates and enjoyed affluence in both the sense that their risk of sickness and death was lessened and because the necessity for short hours of work meant that they had ample leisure. They needed to limit population growth so that their group would not become so large that it had to divide with two undesirable consequences, the social instability caused by the division, and the resulting long and possibly hazardous journey undertaken by a least one of the resulting splinters. The limitation was carried out by killing or deserting infants, the old, the disabled and the sick. This action had two desirable outcomes: it maintained social coherence by postponing or removing the need to divide the group, and it ensured that, if a long trek had to be made, those undertaking it would not need assistance in moving. He drew heavily on reports of the Australian Aborigines and Lee and DeVore's (1968) *Man the Hunter*, but even more basic to his argument was the work of William Allan (1949, 1965) on land carrying capacity in Africa.

Thirteen years after Wynne-Edwards (1962) began to lead anthropologists into biological evolutionary theory, E.O. Wilson's *Sociobiology: The New Synthesis* (1975) tempted many to go further. This was particularly the case in Hausfater and Hrdy's *Infanticide: Comparative and Evolutionary Perspectives* (1984). There Hrdy and Hausfater (1984, pp. xi, xxx) wrote: 'in many populations infanticide is a normal and individually adaptive behaviour', for 'an infant may be eliminated if the parent or step-parent thereby enhances overall reproductive prospects'. In the same book Daly and Wilson (1984) wrote: 'Human infanticide is widespread and a sociobiological model of the human psyche helps to make it intelligible' (p. 502), but cautioned that 'Parentally instigated infanticide that does not make reproductive sense ... will nowhere be described as normal or typical' (p. 489).

## How good is the evidence?

The demographic data are relatively good, but severely restricted. They are restricted in time because most demographers became interested in the fertility of developing countries only when their rapid post-World War II population growth became apparent. The first francophone demographic surveys extend back only to 1954–5 and the first World Fertility Survey to 1975. It is generally agreed among anthropologists that by this time precontact groups uninhibitedly practising infanticide had largely disappeared. The only earlier surveys are the Indian Census starting in the early 1870s and the Buck survey of rural China around 1930 (see Notestein 1938). These two sources indicated by their unbalanced sex ratios either female-selective infanticide or relative neglect of daughters among some groups at certain times (Visaria 1967; Pakrasi 1970; Miller 1981; Lavelly *et al.* 1990). The second restriction is that demographic surveys provided a thin cover (typically 5,000 female respondents) of large national populations, so that small hunter-gatherer populations were insufficiently represented when they were not excluded altogether. The third restriction is that the surveys did not ask questions about infanticide which the law in nearly every country regarded by this time as tantamount to murder. The fourth restriction is their lack of anthropological field work of the kind that might hear rumours of infanticide or stumble on actual evidence. Nevertheless, the wide prevalence of high birth rates, the frequency of the natural fertility distribution of births, and the often attested lack of knowledge of any method for controlling family size provided fairly credible evidence that the new family planning programs were not merely replacing older methods of control.

The anthropological testimony is copious but is dependent on a remarkably small, and probably insecure, base. Much is mainly theoretical and drawn from others' ethnography: Weyer (1962) on Eskimos from Boas (1907), Stefansson (1914), and Jenness (1922) as well as his own work; Douglas (1966) from Rasmussen (1931) and Balikci (1967) on Eskimos, Firth (1936) on Tikopia and Yalman (1963) on the Nambudiris of Kerala; Balikci (1967) from Weyer (1962) and Balikci (1970) from Rasmussen (1931, 1932) on Eskimos; Polgar (1971b) from Birdsell (1968), Devereux (1955), and Firth (1936); Abernethy (1979) from Lindenbaum (1972) on the Papua New Guinea Highlands and Freeman (1971) on Eskimos; and Scrimshaw (1983) from Divale and Harris (1976).

The basic research is of two types, fieldwork and global surveys of data. Fieldwork is dominated by that on Eskimos, and the belief in age-old population control depends to a very considerable extent on the Eskimo evidence, particularly original data reported by Smith (1902), Boas (1907), Jenness (1922), Birket-Smith (1929) and Rasmussen (1931, 1932). These data were not based on longitudinal vital registration but on counts made during visits, not always by the author reporting on them. The most detailed are by Jenness (1922) from the 1914–16 Canadian Arctic Expedition and by Birket-Smith (1929) and Rasmussen (1931, 1932) from the 1921–1924 Fifth Thule Expedition. The quantitative demonstration of the practice of infanticide came almost wholly from age-specific sex ratios which demographers have found elsewhere (Africa, Indonesia, anywhere where women marry young and knowledge of exact age is culturally unimportant) tend to be artificially male-dominated in adolescence because survey- or census-takers have assumed teenage mothers to be at least 20 years of age (see van de Walle 1968a, pp. 44–52). Thus population under 20 may appear to be male-dominated compared with the population over that age. Schrire and Steiger (1974) carried out a detailed analysis of those primary data on the Eskimos and concluded that the statistical information would not support the conclusion that female infanticide was on a significant scale. The only direct evidence they could find of infanticide in over 30 key publications, including all those ever cited, was a single case described by Jenness (1922, p. 166) as happening in 1915 (Schrire and Steiger 1974, p. 164). Their simulation models of these early twentieth-century Eskimo populations showed that any level of female infanticide above eight per cent would have led not to a better distribution of resources but to the extinction of the population

(Schrire and Steiger 1974, pp. 170ff.). Their 1974 paper, highly significant until and unless it can be refuted, and available in the easily accessed journal *Man*, was published before much of the recent theoretical discourse but has rarely been referenced and never seriously discussed. Krupnik (1993, p. 224), in his book on Eskimo adaptation to life in Northern Asia and Europe, concluded that among these people infanticide appears to have been occasionally resorted to in an unusual crisis but not on such a scale that it had any demographic impact.

The second most important ethnographic source has been the Australian Aborigines (Spencer and Gillen 1938; Birdsell 1968; Rose 1968). Spencer and Gillen (1938 [1899]) have been much quoted because they were reporting on the largely precontact Aborigines of Central Australia in the late nineteenth century. They reported that when infanticide was practised it usually merely forestalled an inevitable death: 'Infanticide is undoubtedly practised, but, except on rare occasions, the child is killed immediately on birth, and then only when the mother is, or thinks she is, unable to rear it owing to there being a young child whom she is still feeding, and with them suckling is continued for it may be several years' (p. 51). The third most frequently cited source is Firth (1936) on the Tikopia, but his approach was so impressionistic that little can be said about the incidence of infanticide in an older Tikopia. He thought it had occurred during famines or in the case of illegitimacy (p. 415).

Central to the recent discussion of infanticide is the evidence in Lee and DeVore's 1968 landmark volume, *Man the Hunter*. The most quoted chapter on infanticide is that by Birdsell (1968). This is a short piece of work which is difficult to assess partly because the empirical report is embedded in models and theoretical discussion, and partly because that report refers for its factual content to three manuscripts held by the University of California at Los Angeles. In the same volume Rose (1968) reported on the largely precontact Aboriginal population of Groote Eylandt (in northern Australia's Gulf of Carpentaria) in 1941. He reported that the population was adequately fed, with food collection requiring only three or four hours per day, but that, nevertheless, because of extremely high under-five mortality, the population was stationary, and there was a preoccupation with fertility rites (as Notestein 1945 would have predicted). There was no contraception and little infanticide, and birth and death rates were both very high and approximately equal (Rose 1968, p. 203).

Several other sources have also been important. Neel (1970) a geneticist, very conscious of the importance of the lessons from the past for late twentieth-century population control, reported on a multidisciplinary study of three ethnic groups in northern Brazil and southern Venezuela: 'An accurate estimate of the frequency of infanticide still eludes us, but, from the sex-ratio imbalance [of the population believed to be under 15 years of age] plus other fragmentary information, we calculate that it involves perhaps 15 to 20 per cent of all live births' (Neel 1970, p. 816). Schiefenhövel (1984) reported on infanticide among a precontact population in the Highlands of Irian Jaya (the western half of the island of New Guinea). His wife was one of the few outside observers anywhere in the world to witness an act of infanticide (p. 170). He relied on two sources, sex ratios especially of the population supposedly under 20 years and informants who recalled infanticide in 10 per cent of all live births (pp. 175–176, 181). He calculated for two periods levels of infanticide of 19 and 43 per cent, nevertheless reporting a stationary rather than a declining population (p. 181).

The large-scale surveys of abortion in premodern societies tended to confirm among anthropologists that it had not been an important mechanism for pretransitional fertility control. The most comprehensive, Devereux's (1955) study of abortion in 400 societies, concluded that 'Statistical data on abortion in primitive society are mostly either unreliable or else both unreliable and skimpy' (p. 25). Nag (1968, p. 187) decided from his survey of factors affecting fertility in

nonindustrial societies that the level of abortion in such societies appeared to be much lower than in industrial ones, partly because of the great danger to the women. Similarly, Noonan (1970) summarized the historical volume on abortion he edited as proving that abortion had been known since before classical times but that there was little certain information on its incidence.

Divale and Harris (1976, pp. 533–535) came closest to a quantitative report when they divided 112 societies in the Yale Human Relations Area Files (which originated in the work of Murdock 1967) into 41 per cent with no information on infanticide, 9 per cent where it was not practised, 2 per cent not common, 12 per cent occasional, and 36 per cent common. Most of the reports in the Files are drawn from general ethnographic accounts where the classification of any specific behaviour in a society can be based on a few passing sentences, often impressionistic. Scrimshaw (1983) drew on Divale and Harris's (1976) work but concluded that the impact of infanticide was unknown except perhaps in the differential mortality of daughters (Scrimshaw 1983, p. 257). Piers's *Infanticide* (1978) contributes practically no quantitative information. Williamson's influential 1978 article, 'Infanticide: an anthropological analysis', was quite definite in the conclusions about infanticide: 'Rather than being an exception, then, it is the rule' (p. 61); it is obvious in the classical world, in Imperial China, and among Eskimos and Australian Aborigines (p. 62); 'During the Palaeolithic period, which comprises almost 99 per cent of human history, infanticide was probably universal ... estimated at 15 to 50 per cent of all births' (p. 66); and even in Europe little attempt was made to control infanticide until the late nineteenth century (p. 69). Williamson's sources for these conclusions were Firth (1936), Birdsell (1968), Neel (1970), and Langer (1974a). The 15–50 per cent range is derived from Birdsell's (1968, p. 239) conclusion, and there it is unclear how closely it is related to his references to other studies or to his models.

## Discussion

Most cultural anthropologists who have made known their views on premodern population control have concluded that it was sufficiently great to significantly reduce fertility and mortality (assuming we do not count each incident as a birth and a death), to increase life expectancy beyond infancy, and to raise standards of living, at least in terms of nutrition, health and leisure. There has been far more theory than evidence, although a great deal of cross-referencing has given the impression of a substantial edifice on strong foundations. The core of the theory has been provided by Carr-Saunders (1922) and Wynne-Edwards (1962), and in addition for a minority by Wilson (1975). There is a danger for social scientists in proceeding to incorporate Wynne-Edwards, for he includes man with all other animals which he believes achieve homeostasis by such unconscious means as varying the quota of breeders or the number of eggs released, reabsorbing embryos, and limited survival of the newborn (Wynne-Edwards 1962, p. 9). It can easily be argued that the parallel in human beings is that they have long been endowed with such characteristics in the form of singleton births, postpartum amenorrhoea, even longer amenorrhoea when suckling persists, and high infant mortality.

There are even dangers in following Carr-Saunders (1922) too far because he includes Africans in his races of splendid physique (pp. 234–235) although it is now agreed that infanticide is rare south of the Sahara, and that significant numbers of Africans are not protected by long periods of postpartum sexual abstinence and may never have been (see Schoenmaeckers *et al.* 1981). And Rose's (1968) usually well-fed and comfortably living population of Groote Eylandt did not normally practise population control. There is an irony in posing 'intelligent, well-thought-out infanticide' against 'miserable, unplanned, natural high infant mortality'. Both are forms of mortality, neither reduces the number of pregnancies and births, and both have the same effect in reducing the pressure on resources. Indeed infanticide is often practised against sick babies, those

born soon after the previous sibling, and ex-nuptial births, precisely those likely to die in any case. Thus it is debatable just how much infanticide actually raises infant mortality.

There are several problems in too easily accepting the thesis of universal hunter-gatherer infanticide at levels high enough to limit population growth. The first is that the evidential base is too insecure. It has had to rely on debatable Eskimo data of the early twentieth century to far too great an extent, and almost absurdly on that on the Netsilik Eskimos, a small group living dangerously in an area which has to be evacuated in winter and which provides a diet largely from sea mammals. And models such as those of Birdsell (1968) collapse when confronted with the impossibility of distinguishing between the impacts of infanticide and natural infant mortality. Finally, the apparent evidential structure has been built up by referencing other writings which in turn are based on further referencing or argument based on the little field evidence available.

More importantly, none of these approaches shows how long-term population equilibrium is achieved except ultimately by Malthusian limits where mortality is the final arbiter. It appears likely that in any given set of stable ecological conditions a range of fertility-mortality equilibrium points extend, if we extrapolate from Coale and Demeny (1966, West Model), from a total fertility rate of 7.5 with a life expectancy of 18 years, approximately the levels found in the Mopti district of Mali in 1957–58 (Coale and Lorimer 1968, p. 157), to a total fertility rate of four and a life expectancy of about 33 years, approximately the levels found in England during the century 1650–1750 when growth was almost stationary (Wrigley and Schofield 1981, p. 530). The differences between the two stationary populations were mostly in child mortality, 60 per cent of the former and 35 per cent of the latter dying by five years of age. But those who reached 20 years of age lived on average to 48 years in the former and 55 years in the latter. It was the high-fertility, high-mortality regime that produced the physically splendid Ashanti warriors of a highly organized state, who in the nineteenth century repeatedly drove the British down to the coast of what is now Ghana. Their young children died at horrific rates but the adults did not starve and their state did not disintegrate. The span of Malthusian equilibria just addressed is probably the full range. Natural-fertility total fertility rates go little above 7.5 because of lactational amenorrhoea and other factors. At the other end of the range the constraint was the inability of life expectancy in England to rise to 40 years until the mid-nineteenth century and to 45 years until around 1890 (Keyfitz and Flieger 1968, pp.36–39) when public health interventions were advancing and real average income was three times what it had been two centuries earlier (Maddison 2001, pp. 185, 264).

The Western European marriage pattern, with late or forgone female marriage, and relatively little sexual activity and few births outside marriage, was not developed to reduce societal mortality levels. Its persuasive morality was based on the assertion that men and women did not have a right to marry until they had sufficient means to bring up a family in some comfort. Certainly they knew that this comfort included a greater chance of child survival. It was this lower limit to family misery, and the fact that the poorest often did not marry at all, that reduced child mortality within the family and in aggregate within the society. It was late female marriage and the spinsterhood of many that reduced the societal fertility level without restriction on marital fertility or an aim at such restriction. The Western European population was just as stationary as the Indian population, but the refusal to countenance very low living standards probably meant less pressure to obtain every morsel of food from the land or to bring into production very marginal land, so there probably was less desperate pressure on resources.

This demonstration does not mean that there was no infanticide. Clearly among both hunter-gatherers and some of the agrarian populations of Asia it did occur, especially during subsistence crises and among families with disproportionate numbers of daughters. However, it is not clear that it was on such a large scale as to significantly control population growth. Certainly, populations

were generally stationary, but the Malthusian constraints can satisfactorily explain that situation. Indeed the constraints are needed to explain the population equilibrium even if infanticide is practised on a huge scale or if Boserupian populations occasional burst through food production barriers.

The evidence from contemporary hunter-gatherers and other peoples, of infanticide on a sufficient scale to be the major control on population growth, is not good enough. Infanticide theorists, given a strong lead by Carr-Saunders (1922) and Firth (1936), explained this by the collapse of the old system as a result of Christian penetration and colonial administration, but the proof is not there. The failure of many of the great agricultural populations, such as in most of India, to evidence large-scale infanticide is put down to a hypothesized rise in mortality, and a necessary rise in matching fertility. The rise in mortality is ascribed to a greater post-Neolithic-Revolution density of settlement with a concomitant increase in infectious disease, but little is said about the probable lower mortality level from the violence that characterized hunter-gatherer populations or the fact that infanticide was arguably still a viable means of reducing adult mortality. The lack of infanticide in sub-Saharan African populations, by far the largest non-sedentary agricultural populations with which we have been in contact, is not explained at all. The strongest evidence for massive infanticide comes from Japan (Eng and Smith 1976; Saito 1992), China (Lavelly *et al.* 1990; Lee and Campbell 1994), and parts of North India (Pakrasi 1970) in recent centuries, but even here neither its scale nor its concentration in crisis periods is certain.

Finally, it should be noted that demographers have not all ignored the views of what has been the cultural anthropological mainstream. Wrigley (1978, p. 135) wrote of ‘unconscious rationality’, similar to Adam Smith’s ‘hidden hand’ in the control of fertility (but excluding infanticide). Lesthaeghe (1980) criticized that view on the grounds that reproductive restriction must be consciously socially enforced (pp.529–530), and argued that, if there is ‘homeostasis’ in human populations, it must be achieved by the force of mortality (p. 528). Coale (1986), in the first pages of Coale and Watkins’s (1986) *The Decline of Fertility in Europe*, tried hard to bridge the gap but failed to take into account just how centred was the cultural anthropological thesis on infanticide. Very recently Wilson and Airey (1999) have urged demographers to persist with the effort to derive explanations from the homeostatic approach. This may prove worthwhile provided that there remains scepticism of the thesis of conscious paleolithic population control.

## Notes

John Caldwell is at the Demography and Sociology Program, Research School of Social Sciences, Australian National University, and Bruce Caldwell is at the National Centre for Epidemiology and Population Health, Australian National University, Canberra ACT 0200, Australia.

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